LXVI. *Obſervations on a Clock of Mr.* John Shelton, *made at* St. Helena: *In a Letter to the Right Honourable Lord* Charles Cavendiſh, *Vice-Preſident of the Royal Society, from the Reverend* Nevil Maſkelyne, *M. A. F. R. S.*

My Lord,

Read Jan. 21, 1762.

WHEN we reflect upon the great degree of perfection, to which the ſciences are at preſent brought, and, at the ſame time, conſider from what low beginnings in former times they have ariſen to this hight, we are apt to pleaſe ourſelves with the idea of a certain kind of ſuperiority, which we imagine we enjoy above the learned, who have gone before us. But, when we recollect by what ſlow degrees, and by what painful applications of ingenious men, in various ages, theſe improvements have been brought about, we ſhall, perhaps, be inclined to own, that our fancied ſuperiority over our predeceſſors in ſcience is chiefly that of being ſo fortunate, as, by coming after them, to enjoy the fruits of their labours, joined to our own. But, though we are obliged to give up part of the ſatisfaction, which we might receive, from aſſuming ſomething to ourſelves, from the preſent ſtate of ſcience; yet, in return, we ſhall be gratified with a pleaſure not at all inferior, which the curious mind muſt receive, from this very conſideration of the ſuc-

ceſſive

ceſſive degrees, by which knowledge has been augmented to that pitch, in which we now behold it.

The antients were well acquainted with the rotundity of the earth, and were ſatisfied, that heavy bodies, in every place, had a tendency to its center: but they had never any ſuſpicion, that the force of their tendency to the center was greater in one country than another, or that, when dropped from any hight, they fell faſter in one latitude than another.

The great Huygens, who firſt ſet the doctrine of centrifugal forces in a clearer light, ſaw plainly, that the weight of bodies muſt naturally be leſs at the equator, than at the poles; their great velocity there round the earth's axis taking off part of the weight, which they acquire by their gravitation towards the center of the earth. And, though he was not quite exact in ſettling the true proportion of the force of gravity in different latitudes; yet we owe this obligation to him, of having made the firſt diſcovery of a thing, which hath ſince been the ground of ſo many theories and experiments.

Mr. Richer, when he went to the iſland of Cayenne, made the firſt experimental proof of the decreaſe of gravity, in approaching the equator, though he was not led thereto by Huygens's theory, which was then but lately publiſhed, and not ſo generally known; but, from finding his clock, which he had brought with him, go conſiderably ſlower than it had gone in France.

But Sir Iſaac Newton, firſt of all, ſhewed, how the variation of gravity in different latitudes depended not only upon the centrifugal force, but alſo upon the figure of the earth, which he likewiſe determined, as

well

well as the proportion of the force of gravity in different latitudes, as far as theory alone could limit them.

The ingenious Mr. Colin Campbell made a very curious experiment, of the diminution of gravity from London to Jamaica, by means of an excellent clock, made by Mr. Graham, an account of which is given by Dr. Bradley, in N° 432 of the Philofophical Tranfactions, to which he has added a table of his own, expreffing the proportion of the force of gravity in different latitudes, and has fubjoined the proportion of the equatorial to the polar diameter of the earth, which fhould follow from the experiment, according to Sir Ifaac Newton's principles.

A like experiment was made by the learned French aftronomers, who went to the polar circle, to meafure the length of a degree of the meridian, by a clock made by the fame excellent artift, exprefly for the fame purpofe.

We are likewife obliged to the gentlemen of the Royal Academy of Sciences at Paris, for feveral experiments made by them, in order to determine the force of gravity in different places, by meafuring the length of the fecond pendulum.

I could not fail of being defirous of improving the opportunity, which my voyage to St. Helena afforded me, of examining this curious point, among my other experiments: and your Lordfhip did me the honour, not only of approving of my defign, but alfo of thinking, that it was proper the very beft inftruments yet known fhould be employed for this purpofe; as did alfo the Royal Society, to whom I communicated my intention, who were pleafed to

furniſh me with an excellent clock, with a gridiron-pendulum adapted to it, executed by that diligent and ingenious artiſt Mr. John Shelton.

Dr. Bradley was pleaſed to take upon him the trouble of ſetting the clock up at the Royal Obſervatory at Greenwich, and there to examine its going, where he informed me, that it loſt 11 ſeconds per day, upon ſiderial time, the thermometer of Fahrenheit's conſtruction, which was placed withinſide of the clock-caſe, ſtanding about 50 degrees, at a medium, during the time, in which the experiment was made. Though the contrivance of the gridiron-pendulum might be conſidered as a ſufficient ſecurity againſt any variations in the going of the clock, which might ariſe from the changes of heat and cold, yet it was thought the experiment would be more ſatiſfactory, if the temperature of the air was ſet down at the time.

Soon after my arrival at St. Helena, I ſet up the clock in the valley near James's Fort, in a place elevated 85 feet above the level of the ſea. Being ſenſible, how much the exactneſs of the going of a clock depends upon the firm manner of ſetting it up, I had large pieces of wood driven into a ſtone-wall, between the joining of the ſtones, to which I ſcrewed the back of the clock-caſe, which was very ſolid and heavy, by four ſcrews, the bottom reſting upon a large flat ſtone. The pendulum had not been taken off from the clock, for carriage, but was ſecured to the clock-caſe, in order to prevent it from receiving any damage. A piece of wood was ſcrewed to the back of the clock-caſe, having a round cavity in it before, juſt large enough to receive the bob of the

pendulum

pendulum: another piece of wood, with ſuch another cavity in it, likewiſe fitting the bob of the pendulum, was applied to it, on the fore-part, and ſcrewed firmly to the other piece: ſo that the bob was embraced between the two pieces, and ſecured firmly to them, and to the back of the clock-caſe. Two little pieces of wood likewiſe kept the upper part of the pendulum in its place, from receiving any motion near the center of ſuſpenſion.

When the pieces of wood were taken away, and the pendulum thereby diſengaged, the clock was fit for uſe: only, to adjuſt the pendulum to the ſame exact length, as it was of at Greenwich, a mark had been made on the rod, where the top of the bob of the pendulum roſe to; and Dr. Bradley informed me of the number, which ſtood againſt the index, on the nut at the bottom of the pendulum, by ſcrewing or unſcrewing of which the bob is elevated or depreſſed.

I had a poſt fixed up on the top of a hill, at a diſtance due north of me, to which I always adjuſted the tranſit inſtrument, before an obſervation, which then gave the true time of the tranſits of the Sun and ſtars acroſs the meridian, within a very few ſeconds: for it was not neceſſary for my purpoſe, to be more preciſe in the fixing of my mark, which would have been attended with great trouble, on account of its ſituation, as the clock was to remain fixed up in the place, where it was, only for a few weeks, and as I always adjuſted the inſtrument truely to the ſame vertical circle, which was at the ſame time very near to the meridian.

I ſhall

I ſhall now lay before your Lordſhip the tranſits, which I took of the Sun, contained in the following table; the firſt column of which, expreſſes the day of the month of the obſervation: the ſecond, the time ſhewn by the clock, at the inſtant of the tranſit of the Sun's center, acroſs the middle vertical wire of the inſtrument, on that day: the third column is made from the ſecond, by applying the equation of time thereto. Therefore, the differences between the numbers in the ſecond column, ſhew the rate at which the clock gets upon the Sun; and the differences between the numbers in the third column ſhew the rate, at which the clock gets upon mean time.

	Time by the clock at the Sun's tranſit.			Time by the clock, with the equation of time applied.		
1761.	h	m	ſ	h	m	ſ
April 25.	0	11	30	0	13	47
27.		15	10½		17	48
29.		18	49		21	45,1
30.		20	40½		23	44,8
May 3.		26	16¼		29	42,1
6.		31	56¾		35	39¼
8.		35	45½		39	36,4

The thermometer, placed in the clock-caſe, taking a medium between the obſervations of morning, noon, and night, ſtood each day as follows:

		Degrees.
April	25.	73
	26.	73
	27.	73
	28.	73
	29.	73
	30.	72
May	1.	72
	2.	72
	3.	71
	4.	71
	5.	70
	6.	70
	7.	72
	8.	72

Your Lordſhip will perceive, that, from April 25 to April 27, the clock got $4^m\ 1^s$, upon mean time, in two days; which is at the rate of $2^m\ 0^s\frac{1}{2}$ per day. In like manner, from the differences between the following obſervations, the clock appeared to get per day $1^m\ 58^s\frac{1}{2}$, $1^m\ 59^s,7$, $1^m\ 59^s,1$, $1^m\ 59^s$, $1^m\ 58^s,6$; the mean of all which is $1^m\ 59^s,2$. The thermometer in the clock-caſe, at a medium, during this time, ſtood at 72.

The obſervations, as your Lordſhip will perceive, agree ſo near together, in giving the ſame rate of the going of the clock, that there is ſcarce room for preferring one to another. But, from the circumſtances of the obſervations, I ſhould rather chuſe to deduce the rate, at which the clock gets per day, from the tranſits of April 30 and May 8; which is exactly

1^m

$1^m\ 59^s$; the thermometer, at a medium, during this time, ſtanding at 71.

Allowing, therefore, the clock to get $1^m\ 59^s$ in a mean ſolar day, upon mean ſolar time, in $23^h\ 56^m\ 4^s$ of mean ſolar time, or in a ſidereal day, it will get only $1^m\ 58^s\frac{2}{3}$ upon mean ſolar time: but a clock adjuſted to mean ſolar time loſes $3^m\ 56^s$ by the ſtars, in a ſidereal day, therefore the clock loſes $1^m\ 57^s\frac{1}{3}$ of ſidereal time, in one revolution of the ſtars; which agrees exactly with what I find by the tranſits ε, υ, α, and ζ Leonis. For, April 30, ε Leonis paſſed at $7^h\ 32^m\ 30^s$ by the clock; υ Leonis at $7^h\ 39^m\ 24^s$; α Leonis at $7^h\ 55^m\ 52^s$; and ζ Leonis at $8^h\ 3^m\ 36^s$. And May 8, ε Leonis paſſed at $7^h\ 5^m\ 10^s$; υ Leonis at $7^h\ 12^m\ 3^s$; α Leonis at $7^h\ 28^m\ 31^s$; and ζ Leonis at $7^h\ 36^m\ 15^s$. Therefore, the clock loſes $27^m\ 20^s$ by ε Leonis, in the ſpace of 14 days, and $27^m\ 21^s$ in the ſame time, by the other three ſtars: the medium is $27^m\ 20^s\frac{3}{4}$; whence the clock loſes upon the ſtars, in one revolution, $1^m\ 57^s\frac{1}{3}$; which agrees entirely with what is deduced above from the tranſits of the Sun, which was $1^m\ 57^s\frac{1}{3}$.

The ſame clock at Greenwich, with the pendulum adjuſted to the ſame length, loſt 11^s upon the ſtars, in a ſidereal day. Therefore, the force of gravity at Greenwich is to the force of gravity at St. Helena, as the ſquare of $23^h\ 59^m\ 49^s$ to the ſquare of $23^h\ 58^m\ 2^s\frac{2}{3}$ :: 10000000 : 9975405. The extent of the vibrations of the pendulum here, as well as in England, is exactly $1^\circ\ 45'$ on each ſide of the perpendicular, according to the divided arch, which is at the bottom of the pendulum.

 I flatter

I flatter myſelf, your Lordſhip will excuſe me, if I do not attempt to deduce any conſequences, at preſent, from the above obſervations, either with reſpect to the law, which the force of gravity obſerves in its changes in different latitudes, or with reſpect to the figure of the Earth, which it has been ſuppoſed might be determined from experiments of this kind alone, independently of any others, the great Sir Iſaac Newton having himſelf ſet us the example. If the body of the Earth was homogeneous throughout, not only the figure of the Earth, but alſo the law of the variations of gravity in different latitudes would be given, and would be the ſame as Sir Iſaac Newton has deſcribed them. But if the Earth be not homogeneous, and there ſeems great reaſon, from late experiments, to doubt if it be ſo, we can form no certain concluſions concerning the figure of the Earth, from knowing the force of gravity in different latitudes; as this force muſt depend not only on the external figure, but alſo in the internal conſtitution and denſity of the Earth.

Your Lordſhip will, perhaps, think, many more experiments, not only of the kind, which I have the honour to give an account of to your Lordſhip, but alſo of other different kinds, to be neceſſary, before we ſhall be able to infer any thing with certainty, concerning the internal conſtitution of the Earth, or even to determine its external figure. But every experiment is uſeful, which tends to throw a light over this intricate ſubject, and to ſhew the perfect agreement of the laws of nature, with the actual conſtitution of things.

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In hopes the experiment here recited may be taken in this view, as tending, in conjunction with a variety of others, to illustrate an important point, I beg leave to present it to your Lordship; and am

My Lord,

Your Lordship's

most obedient

and devoted

humble servant,

St. Helena,
July 30, 1761.

Nevil Maskelyne.

LXVII. *Observations upon some Gems similar to the Tourmalin; by Mr.* Benjamin Wilson, *F. R. S.*

Read Jan. 28, 1762.

THE honour I received from the Royal Society, in consequence of my late papers on electricity, and which principally respected the Tourmalin, is a pleasing motive for embraceing the first opportunity to communicate farther enquiries, that tend to throw more light upon this curious subject.

In September last, I met with several gems of different sizes and colours, that resemble the Tourmalin, in regard to electric experiments. The most beautiful of